

THE JOHNS HOPKINS UNIVERSITY



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Professor Joshua Lederberg, The Rockefeller University,
66th Street & York Avenue, New York, New York, 10021

Dear Professor Lederberg,

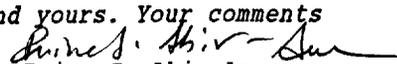
January 28, 1991

I hope my letter finds you well. I also hope that Yaron Ezrahi (my first teacher in science policy) gave you my regards last summer. A propos Yaron's talk at your Carnegie Conference, he said he will give me a copy, but, by his own account, he is not a great correspondent, so perhaps you can tell me when the Conference Proceedings would be available. I thought it might be useful for my current project of transforming a NSF supported workshop at JHU (Program enclosed for your possible interest) into a collection of essays integrating the frontier in science policy with that of research on women in science, while addressing the problem of women's underrepresentation in science.

As you may know, the joint meeting of the US-HSS and its British counterpart will take place this July (25-28) in Toronto. At the request of the Program Chair, Prof. Alan Rocke of CWR, I am organizing a session on "History, social construction and policy implications of molecular biology". Would you be willing to join it with a talk on the role of microbial geneticists such as yourself, E. Tatum and G. Beadle, in becoming spokesmen for the yet undifferentiated and unestablished field of molecular biology, especially in the late 1950s, or prior to the impact of m-RNA, the operon, or the genetic code? I refer to enthusiastic support such as your "M.B., E. & Euphenics" in Nature 1963, among others initiatives. If you are able to join this session, which I hope to dedicate to John Edsall's 90th Birthday, (he has been a mentor for 15 years!) please send a tentative title (& Fax/ phone numbers) by February 7, since the Program Chair needs the whole session ready by February 10, for inclusion in the April issue of HSS Newsletter.

Last, please accept my belated thanks for the list of NAS couples which you so generously sent to me, after our meeting at the Boston Univ. Symposium last April, re: the collection of essays on Creative couples in Science, that I am co-editing with Profs. Pycior and Slack. I appreciate your help but unlike our common colleague and friend Harriet, for whom a scientist's placement in the reward system is a key criterion in selecting her population, I tend to work only on those men or women of science whose work pertains to the history of molecular life sciences. Therefore, I wonder whether you might reconsider your objection that I include the collaboration between you and E. Lederberg in the 1940s and 1950s in my projected essay on "Collaborative couples in the early days of molecular biology"; without an essay of my own, my position as co-editor is being jeopardized (since our co-editors serve as role models for other authors). You were the most important collaborative couple at the time; without your cooperation my essay will not be worthwhile doing (even though I plan to include the Staniers, the Gorinis, and the Ephrussis for cross-national purposes). Kindly, let me know what else I can do to persuade you that this project, limited to a period prior to the 1960s, should be allowed to start. My co-editors and I await your decision with eagerness.

With best wishes for a healthy and happy 1992 to you and yours. Your comments on the enclosed reprints are very welcome. Sy


Dr. Pnina G. Abir-Am

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